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THE BOTURINI-AUBIN-GOUPIL COLLECTION OF MEXICANA.[1]

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IN the year 1736, an Italian of long lineage but light purse landed at Vera Cruz His name was Lorenzo Boturini Benaduci, and the business which took him to Mexico was the collection of the arrears of a pension due some of the descendants of Montezuma who then resided in Portugal. This naturally led him to a study of the native history of Mexico, a pastime which soon grew to an enthusiasm, when he learned that the Blessed Virgin herself had appeared and talked with a poor Indian on the hill of Guadalupe. Fired by a noble frenzy, he decided to devote his whole life to these two objects,—the collection of every document which would throw light on the ancient history of the indigenous population, and the vindication of the apparition of Our Lady of Guadalupe.

To these aims he gave up nine consecutive years, and all the money that he could borrow or beg; for his own supply of that useful article was uncomfortably limited. But a foreigner, a begging foreigner, and that foreigner an archæologist, was a combination too repugnant to the Spanish constitution to be stomached long; so, in 1745, the vice-regal government seized Boturini, threw him into prison, and sequestrated his collections of books and manuscripts, so precious in his eyes, as he pathetically wrote, "That I would not exchange them for gold, nor silver, nor diamonds, nor pearls." How the true spirit of the collector breathes in those lines! But, alas! he was destined never to see them again. Removed from prison, he was sent to Spain for trial, where he died in 1749. His priceless collection was presented by the viceroy to the University of Mexico, whence it was scattered to different private and public owners.

Boturini was born in 1702. Precisely a century later, J. M. A. Aubin was christened, in a little town in the south of France. He was destined to partake of the same divine antiquarian fervor, and to re-collect for all posterity the scattered jewels of his predecessor's cabinet. With a liberal fortune and the best of introductions, he resided in Mexico from 1830 to 1840, and with singular tact and energy succeeded in securing a large part, and the best part, of the documents gathered with such toil by the Italian antiquary. He brought them to Paris, where he lived surrounded by them for fifty years, making very little use of them himself, and never permitting a single student so much as to look at them. Why this misanthropic narrowness? The reply should be guarded. A cloud hung over Aubin's lonely life. He, too, was imprisoned; as he claimed, by malignant enemies; but on a charge which forever blasts a life. I even heard indignant protests at his mere presence, when poor, old, and senile, he was led into the hall of the Congress of Americanists, in 1890, at Paris.

Enough of this sad subject. At any rate, M. Aubin merits the lasting gratitude of investigators that he preserved with scrupulous care his wonderful collection. When I saw him in 1890, it was no longer his. Financially ruined by investments in "Panama," he had accepted an offer for the whole of it from M. Eugene Goupil, a native of Mexico, French on the paternal side, tinctured with the blood of the native race through his mother. He bought it, not as an antiquary, but as an enlightened lover of his country and an intelligent patron of antiquarian studies. He placed the manuscripts in native tongues, Spanish or Latin, the wondrous colored pictographic scrolls on maguey paper, or on skins of animals, the ancient Codices, maps, and titles, in the hands of M. Eugene Boban, a distinguished antiquary, well known in the cities of Mexico, New York, and Paris, from his long residence in them all. To his kindness I owe the privilege, enjoyed by few, of a leisurely inspection of this wholly unrivalled collection of Mexicana.

M. Boban's task was to make an analytical catalogue of the three hundred and seventy-two pieces of which the collection consists. He has completed that task in a manner in the highest degree creditable to his own scholarship and to the discriminating liberality of M. Goupil. His work is comprised in two very large quarto volumes of text, together of more than a thousand pages; and a third thick volume or atlas, containing photographic reproductions of some of the most remarkable documents. Yet this huge publication is but the mere beginning of the labor which must be expended on this mass of material before its value is extracted. As for myself, after seeing what it contains, I made up my mind that all that has yet been written about Mexico previous to the conquest has no more importance than have the histories and descriptions of ancient Egypt which were composed before the method of hieroglyphic interpretation was discovered.

The title of M. Boban's work is:—

"Documents pour servir à l'Histoire du Mexique. Catalogue Raisonné de la Collection de M. E. Eugene Goupil (Ancienne Collection J. M. A. Aubin)." Paris, Ernest Leroux, Editeur, 1892. Price, 180 francs.

The first volume begins the catalogue with the celebrated *Historia Chichimeca*, an ancient Codex on agave paper, painted in blue, green, and brown, and giving in hieroglyphic characters the history of pre-Columbian Mexico, from A.D. 963 to 1428. It was translated by the early native chronicler, Ixtlilxochitl, and for that reason all the ten leaves of which it consists are reproduced in phototype with the explanations. Following this, a full description is given of what are called the "Maps" of Tlotzin, Quinatzin, and Tepechpan, long pictorial scrolls, partly published by M. Aubin, relating the migrations and traditional history of the Nahuas. Next comes the curious *Codex Cruciformis*, an original, painted, figurative manuscript relating to Tezcuco and Tenochtitlan. It is painted in four quarters, of thirteen compartments each, somewhat like a Maltese cross, whence the name given it.

The famous *Tonalamatl*, or "Book of Days," is then taken up. This is an original, hieroglyphic book of eighteen leaves, magnificently colored in red, black, green, and brown. Its purpose is that of a religious and divinatory calendar, serving at once as a ritual and as the basis for astrological prognostics. None of the documents in the collection presents to the eye a more striking appearance than this venerable pictograph, concealing under its strange and vivid coloring the dark wisdom of the Aztec diviners.

Relating to the same subject, perhaps, is a remarkable painting on a tanned deer-skin, representing a disk with fifty-two points, that being the number of years in a Mexican cycle. A phototype is given of this, and M. Boban thinks it is intended to prescribe days for the worship of the sun, *Tonatiuh;* but it is more likely to be simply the computation of a calendar.

Another historical pictograph is the *Codex Mexicanus*, an original, of forty-seven leaves, narrating the history of the Mexicans from their departure from the mysterious land of Aztlan down to the year 1590. This is native work, though late in the sixteenth century. The *Codex de Vergara* is another figurative document, defining boundaries and titles, whose date is 1528. Like many of these title deeds, it contains valuable hints as to the nature of the Mexican pictographic system.

The collection is peculiarly rich in books written in the Nahuat language. There are the *Historia Tolteco-Chichimeca*, the *His*

[1] Read before the Numismatic and Antiquarian Society of Philadelphia, March 2, 1893.

toria de Tlascala, the *Codex Chimalpopoca*, the *Anales de Cuauhtitlan*, manuscripts of Ixtlilxochitl, Leon y Gama, Father Pichardo, and others. Very curious are the catechisms of the early missionaries written in the Mexican hieroglyphic characters, the maps, charts, plans, "Titulos de Tierra," legal documents, and royal ordinances, throwing light on the early history and settlement of the territory of Mexico.

M. Boban concludes his long and arduous task by adding a comprehensive and well-arranged index to his volumes; and I should not omit to mention that he increases the practical value of his work by inserting a series of biographical notices and many quotations and references to contemporary Mexican archæological literature.

I have reserved the best piece of news to the last. I learn from good authority that it is the intention of the enlightened M. Goupil finally to concede to scholars the access to this marvellous storehouse of American antiquity by placing it in the possession of the Manuscript Department of the Bibliothèque Nationale. Certainly no one in this generation will more deservedly receive the thanks of all genuine Americanists than the donor of such a treasure to public use.

TIME-PERIODS OF THE MAYAS.

BY PROFESSOR CYRUS THOMAS, WASHINGTON, D.C.

IN "Current Notes on Anthropology," *Science*, Feb. 10, reference is made by Dr. Brinton to the article on "Time-Periods of the Mayas," by Dr. Förstemann, in *Globus* (Bd. 63. No. 2). In closing this notice, he remarks that "Dr. Förstemann's discussion of the subject amounts to a demonstration,"—an asserton I think he will find it difficult to maintain. I presume, however, it was based on Dr. Förstemann's well-known ability as an investigator in this line, his long and faithful study of the time-symbols of the Maya Codices, and his great caution in presenting conclusions, rather than on a thorough examination of the data.

I am indebted to Dr. Förstemann for several valuable suggestions in my work in this line; it was through one of these, given in a private communication, that I was led to the evidence on which I base some of the objections offered here to his conclusions.

He believes that the different steps by which the Mayas reached their final calendar with the year of 365 days, consisting of 18 months of 20 days each, were as follows: First, the period of 20 days, next the period of 18 months, giving the year of 360 days; next, the year of 364 days, formed by adding four days at the end of the eighteenth month, at which time the division into periods of 13 days was introduced; and, finally, the year of 365 days, by adding another day at the end of the eighteenth month. The evidence on which this is based he believes he finds in the Codices, chiefly in the Dresden Codex. He believes he finds evidence of the use of all these years, as also of the Tonalamatl or Sacred year of 260 days in the latter Codex.

We take first his basal or cyclical period:—

$$\left.\begin{matrix}1\\19\\0\\0\end{matrix}\right\}$$ or 14040 days, found in the right column of Dres., p. 73.

There is no doubt that this denotes, as he contends, 14040 days, or 39 years, if we count 360 days to the year. "From this," he adds, "proceed two series, of which one has the difference 65, . . . while the other increases by 54." He alludes to the series running through the upper division of pp. 71–73, where the difference is 54; and that running through the middle and lower divisions of the same plates, where the difference is 65 (see our "Aids to the Study of the Maya Codices," pp. 332–337). It is to be noticed, however, that there is no connection between his typical number and these series, and why he has thus referred to them is not apparent. On the contrary, it appears from the 9 Ix below it to belong to the right-hand series of the upper division. I also made the mistake in my "Aids" (p. 337, note) of connecting this 9 Ix with one of the series mentioned.

The point he makes is, that this number is divisible by 360, and that the two series referred to can be explained on this theory, hence it is presumable a year of this length was used in constructing them. Now it must be conceded that if these series can be explained and traced out in accordance with the usual calendar of 365 days to the year, and the four year-series, Dr. Förstemann's argument loses its force, and falls short of a "demonstration."

Let us see if this can be done. For this purpose we present here a part of the series in the middle division of the plates alluded to.

1						
4	19	16	13	9	6	3
15	10	5	0	15	10	5
4 Manik	4 Ik	4 Caban	4 Eb	4 Manik	4 Ik	4 Caban.

This series, which begins with the number and day at the right, ascends, and is to be read from right to left, the difference being 65 days, or 3 months and 5 days, if the numbers are intended to denote days, months, and years. The 19 in the 6th, or next to the left-hand column, is evidently the same as 1 unit of the third order and one of the second, or 1 year, 1 month (counting 360 days to the year). If the year contained only 360 days, it must have commenced year after year with the same day unless there was an arbitrary change. On this theory the numbers in the lower line of numerals (with one exception) might denote the day of the month. For example, Caban would be the 5th day of the month if the year began with Ben, or with Ix counting from the last day of the month; Ik the 10th, Manik the 15th, and so on through the entire series, and also in numerous other series. This would seem to be a sufficient "demonstration" of the theory, and was considered so by me in my "Aids," but the numeral system in the Maya calendar is exceedingly deceptive. Before this is conceded, it is necessary to overcome the following objections: The figures in the middle row do not give the months correctly nor those in the upper the years. The 3, 5, in the first column, really donote the 5th day of the 4th month. While the 1 in the left-hand column, if taken in this way, would refer to the second year. Moreover, if the numbers in the "month" and "day lines" were intended to denote the numbers of the months and days of the months there could be no blanks, such as we see in the 4th column above ($\begin{matrix}13\\0\end{matrix}$). That the symbol represented by the cipher signifies "nothing," is admitted by Dr. Förstemann, and is proven by the number in the month line. As upon the theory of 360 days to the year, all the years should begin with the same day, while this method of counting time remained in vogue, the different series based upon this method should be referred to years commencing with the same day. This, however, is not the case, as the series now under consideration pertains to a year commencing with Ben; while the long series on pp. 52–58 can be reckoned only in years beginning with Lamat. Nor is it possible to bring these series into harmony in this respect upon the theory of a year of 360 days unless we assume there were arbitrary changes, which amounts to begging the question. It is also inconsistent with this theory that the series on pp. 63–64, which Dr. Förstemann believes to be founded on the year of 364 days, gives precisely the same results in the respect mentioned as the other series referred to. In truth, it is impossible that the "day" and "month lines" of numerals should indicate the days of the month and numbers of the months throughout a series extending over several years, except upon the theory of 360 days to the year. We are forced, therefore, to the conclusion, even on Dr. Förstemann's theory, that these series are only successions of intervals in which the columns of numerals simply denote the sum of these intervals at the various steps.

We will now proceed in our attempt to explain the series on pp. 71–73, of which a portion is given above, by the usual calendar system of 365 days to the year and the four year-series. No difference between the two systems will appear until we reach the end of the first year of the series. As this is reached in passing from the 5th to the 6th column, $\left\{\begin{matrix}16\\5\\4\text{ Caban}\end{matrix}\right.$ and $\left\{\begin{matrix}19\\10\\4\text{ Ik},\end{matrix}\right.$ we start with 4 Caban of the 5th column. As before stated, this series proceeds from right to left and is to be counted from the

last day of the month — or these days considered as the first of the month, as Dr. Seler concludes As Caban, counting in this way, is the 5th day of the month in Ben (Ix) years, we take it as 16 our starting point. As the figures in this column (5) show that 16 months and 5 days, or 325 days, have been counted up to this point, our 4 Caban must be the 5th day of the 17th month. It follows from this that the starting-point of the series is 5 Ben and that the year is 5 Ben (or 5 Ix, counting from the last day of the preceding month). If there are 365 days in a year there will be 40 days (out of the 65) to count in this year and 25 in the next. As the year (including the 5 added days) will end on 5 Caban, the next will begin with 6 Ezanab (a Cauac year). Counting forward 25 days in this year, we reach 4 Ik, which is the day under the 6th column, but it is the 5th and not the 10th day of the month. This is not an accidental hit, but has been found true in all these series so far as I have tested them, except that in some the months begin with the usual days as the series on pp. 63-64.

But this is not all, the same result will be obtained in the series we are examining if we start with 4 Caban of either of the other three years, except that 4 Ik in the Kan (Akbal) years will fall on the 20th day of the month, in Muluc (Lamat) years on the 15th, and in Ix (Ben) years on the 10th.

It follows, therefore, that these series can be traced and explained as well upon the theory of 365 days to the year as 360. That the series on pp. 46–50 can only be followed out on the usual calendar system is admitted by Dr. Förstemann, and it was through a suggestion he kindly gave me a year or two ago that I was induced to examine it on this theory.

Is it therefore legitimate, in view of these conflicting results based upon the Codex and Calendar, to say that Dr. Förstemann's discussion "amounts to a demonstration?" Does not what has been shown do away with his conclusions so far as they are based upon the supposed year of 360 days? If all the series susceptible of being tested can be explained satisfactorily in conformity with the usual Calendar, is there any necessity of resorting to any other theory?

It is somewhat strange that Dr. Förstemann should consider the series we have been referring to, the sum total of which is $\left\{\begin{matrix}5\\1\\0\end{matrix}\right.$ or 1820 days, as based on the year of 360 days; and yet refer that on pp. 63–64, which has precisely the same sum total, to the year of 364 days. Both are divisible by 364 and neither by 360, and the numerals in both are given on the same plan, the only difference being that in one case the intervals are 65 and in the other 91. Is this a sufficient basis upon which to found the theory of such a radical change in the calendar system? Yet it seems to be the only foundation for this conclusion. That there must have been steps of improvement in the calendar to bring it nearer and nearer the true year is admitted, but is it likely that these various stages of progress showing years of different length will be found in one and the same Codex? It is only necessary to state that this series can also be counted by the usual calendar. In speaking of the divisors of 364, Dr. Förstemann says: "The number 364 is, however, not merely equal to 4×91, but also 28×13, and this seems to have been the cause of the year being divided into periods of 13 days, as the period of 20 days was a natural divisor of 360 days." As the steps in the formation of the calendar indicate periods of usage of the different years, we must conclude, if this supposition be correct, that the division of the year into periods of 13 days was not in vogue during the time the year of 360 days was in use. Nevertheless, we see by the red numerals attached to the days that it is used in connection with the series on pp. 71-73, which he thinks is based on the year of 360 days. In this we have another illustration of the objections which present themselves to the supposition that years of different length were used in the same calendar.

There is another consideration which, according to the opinion accepted by most archæologists, stands opposed to the idea that the year of 360 days should be found in the Dresden Codex. It is that the time-system used on the Palenque "Tablet of the Cross" is that of the usual calendar except that the count is from the days usually given as the last of the month. This is susceptible of proof beyond any reasonable doubt. If, as is generally supposed, this tablet is one of the oldest records remaining in which calendar dates are used, and antidates the Dresden Codex, is it probable we shall find an older year in the latter?

Dr. Förstemann's suggestion that the series on p. 24 and pp. 46–50, especially those on the latter plates, refer to the revolutions of the planet Venus, appears to rest upon a surer foundation than his theory in regard to the year of 360 days. It is a singular fact that the series on p. 24 is divided into periods of $\left\{\begin{matrix}8\\2\\0\end{matrix}\right.$ or 2920 days, which is an exact multiple of 584; and that the series on pp 46–50 is not only divided into periods of 2920 days, but these are subdivided into periods of 584 days. As will be seen by referring to the plates of the Codex 46–50 or to my "Aids" (p. 298), the red counters at the bottom of each of the five plates are $\begin{matrix}11 & 4 & 12 & 0\\16 & 10 & 10 & 8\end{matrix}$ or 236, 90, 250, and 8, the sum of which is 584, the length of the apparent revolution of the planet Venus. As the numeral series (the word "numeral" is used specially here) runs through five pages, the period 584 being repeated in each, we have a total of $\left\{\begin{matrix}8\\2\\0\end{matrix}\right.$ or 2920 days. But the "numeral series" is only one-thirteenth part of the entire series, for when one horizontal *line* of the day columns at the top has been traced through the five pages to its end on p. 50, we return to p. 46 and trace the second *line* through, for they connect according to the red counters, and so continue until we have traced the thirteen lines ending with 1 Ahau, the lower right-hand day-symbol on p. 50. Thus we see that the entire series embraces a period of 37960 days, or exactly 104 years of 365 days, a fact noticed by Dr. Förstemann. Yet this is not all that we find in this respect on these five plates. They contain two other precisely similar series. The one which has been referred to is based on and relates only to the month symbols which form the upper line of the text in the middle division; the next, using the same series of days and numerals, is connected with the month symbols forming the upper line of the text in the lower division, and the third with the month symbols in the lower line of the lower division. Dr. Förstemann also alludes to these three series. As each series embraces 104 years, we might suppose the three together to form one great cycle, or Ahau-Katun, of 312 years, but, unfortunately, there seems to be no other connection between them than that they are divided into the same intervals and same days. This is evident from the fact that the upper series (not counting back the 11 months and 16 days with which it begins) commences with 3 Cib, the 4th day of the month Yaxkin in the year 11 Ben (or 11 Ix, counting from the last day of the month); the second or middle series from 3 Cib, the 8th day of the month Zac in the year 4 Muluc (or Lamat);[1] the last or lower series with 3 Cib, the 19th day of the year 4 Ezanab (or 4 Cauac, counting from the last day of the month).

If we count back 11 months 16 days from the first date given in each series, thus reaching the initial day, the following singular result is obtained: the first is found to commence with 3 Yimx, the 13th day of the month Mac in the year 10 Muluc (or 14th day, counting from Lamat); the second, on 2 Yimx, the 18th day of the month Kayab in the year 3 Kan (19th day, counting from Akbal); the third, on 2 Ymix, the third day of the month Xul in the year 4 Cauac (4th day, counting from Ezanab). Therefore, if we arrange them to follow one another in time, we shall find an interval between the first and second of 19 years, and between the second and third of 27 years. It is therefore probable that these three series cover substantially the same period, the dates of the different series falling, in most cases, in different months of the same years; or, in other words, that the periods embraced overlap one another. The great length of the series, and their failure to connect, present the chief reasons for doubting Dr. Förstemann's suggestion in regard to their meaning. On the other hand, there is an oft-repeated glyph in the text which seems

[1] It is strange that the author of the Codex has, in this single instance in all these long series, counted from the 1st day of the month.

to give credit to the theory. Notice of this, however, will be reserved for a subsequent paper.

Attention is called again to the series on pp. 63–64, in order to remark that, by counting back from 13 Ix 91 days, we find that the series commences with the first day of the year 12 Kan. Then, by tracing it through, according to the usual year of 365 days, we find that it ends with 13 Akbal, the last day of the year 3 Kan, omitting the five supplemental days at the end. Adding these five days, the total — 1825 — is exactly divisible by 365. However, it seems that the series should be extended 42 days more to include the other days of the last column (see "Aids," p. 330); in which case neither 365, 364, nor 360 would be an exact divisor of the sum total.

We refer next to Dr. Förstemann's theory that the long series on pp. 51–58 refers to the length of the lunar month. As he admits, the number of days, counting to the last, is 11960, though the sum of the intervals between the columns, as shown by the final numeral, is 11958. These intervals are generally 177 days, but 9 of 148 days occur at nearly equal steps, and 6 of 178 at irregular steps. He finds that by multiplying 29 by 3 and 30 by 3 and adding together the products he obtains 177; that the sum of the products of 29 by 2 and 30 by 3 is 148. To obtain the 178, he finds it necessary to arbitrarily add 1 to the products of 29 by 3 and 30 by 3. Next, he finds that by multiplying 177 by 54 — the number of times this interval occurs in the series — 148 by 9 and 178 by 6, and adding thereto 6, he obtains as the sum of the products 11958. He ascertains in this way that 29 occurs 198 times and that 30 occurs 207 times, making together 405, and that 11958 divided by this sum gives 29.526 days, which falls short of the lunar month but one four-thousandth part of a day. As he adds 6 days to his several products to obtain the number 11958, would it not be as well to add 8 days, making 11960, the true length of the series, which, divided by 405, gives as the quotient 29.53 days, precisely the desired figures?

Notwithstanding my high appreciation of Dr. Förstemann's ability as an investigator, and of his great caution in presenting conclusions, I cannot help thinking that his love for numerical coincidences, created by his long study of the time series of the Dresden Codex, has, in this instance, led him to accept as satisfactory what he would have hesitated to approve had it been presented by any one else. Now, 11960, the true length of the series, embraces precisely 46 periods, or sacred years, of 260 days, so often repeated in the Codices, the whole series and each of these periods commencing with 12 Lamat and ending with 11 Manik, initial and terminal year and month days, according to the method of counting from the last day of the month, which I had not discovered when my "Aids" was written. Is it not, therefore, more reasonable to conclude that the chief relation of the series is to this sacred period? This inquiry is certainly pertinent in view of the fact that neither 29 nor 30 appears singly or in multiple at any point in the series, that the total is first lessened by subtracting 2 and the products increased by the addition of 6. It is proper, however, to admit here that the interval 178, which is an increase by 1 of the usual period of 177 days, is difficult to account for, but such difficulties occur at many points in this Codex, and Dr. Förstemann's attempt at explanation involves so many assumptions as to cause us to hesitate before accepting it.

In order to show the uncertainty of the method adopted in regard to the last mentioned series, we will apply it to one not referred to by Dr. Förstemann, running through the lower division of pp. 30–33. In this case the total sum is 2340 days, and the uniform interval 117. Now if we multiply this interval by 5 we obtain 585, but one day more than the time of the apparent revolution of Venus. Or, if we multiply 584 by 4 and add 4, we obtain 2340, the number of days in the series; and the result is obtained by a less addition than that made by Dr. Förstemann in obtaining the lunar period. Now let us try another experiment in order to find the lunar period, thus: $29 \times 3 + 30 \times 1 = 117$ and 2340 divided by $117 = 20$. This will give us 60 periods of 29 days and 20 of 30 days, and dividing 2340 by 80, the sum total of these, we obtain 29.25 days, lacking only about one-fourth of a day of the correct time. Finally, we observe that 2340 days equal 9 of the sacred years of 260 days each, probably the real basis of the series, as 13 and 20, from which the latter is formed, are both factors here — $9 \times 13 = 117$, $13 \times 20 = 260$, and $260 \times 9 = 2340$.

If we turn to the series on pp. 46–50, in which Dr. Förstemann thinks he finds the Venus period, and apply the method of figuring above alluded to, we shall obtain some curious results. As we have seen, the intervals which together make the 584 days are 236, 90, 250, and 8 days. Are these intervals arbitrary, depending upon arrangements by the priests or by the scribe, or should we infer that they always depend upon the periodicity of certain natural phenomena, and hence form factors or multiples of time-periods? Although the latter may be generally true, the proof of which seems to be the chief object Dr. Förstemann has in view in his mathematical search, yet there are many of the intervals and periods which apparently defy all efforts to fit them into place. That 13, 20, and 18 will most frequently appear is to be expected, as they are always factors, but the coincidences in regard to other supposed time-periods (aside from the ordinary and sacred years) are to be regarded with doubt unless there is something more found than the occasional appearance thereof as factors. For instance, if we take 236, one of the intervals mentioned above, we find that it can readily be made to coincide with the lunar period; thus: $29 \times 4 + 30 \times 4 = 236$. This will give as the time of a revolution 29.5 days, which varies less than an hour from the true period. Yet for all this shall we conclude that here we find allusion to the moon's period? By no means, for this is only a recurring interval; and the others, which go to make up the 584, —the 90, 250, 8, —do not coincide with the moon's revolution or any other known time-period; 90 and 8 are factors of 360, but this number, as we have found, is one of the counters in these series.

The supposition that the revolution of Mercury is indicated by the numerals on p. 24 is certainly based on very slender data. This is found only in the fact that 115, the time of a revolution, is a divisor of the large number 11960, which is a multiple of 260, on which it is doubtless based. Why he has referred in this connection to p. 24 is not apparent. I do not find any relation here between a 1 Ahau and 4 Ahau (the latter is found but once on the page); nor do I find the number alluded to (11960) as the terminus of a series or an interval. There are two series on the page, or one series in which the interval varies. That which occupies the lower three-fifths of the right, commencing at the bottom, running to the left and up, has 2920 as its interval, of which 115 is not a factor. The interval of the other, the terminal columns of which are found at the left below, is 2200. This is not divisible by 115. Therefore, so far as I can see, Dr. Förstemann's only basis for the supposition that the Mayas had ascertained the period of the revolution of Mercury is found in the fact that the large number 11960, which is found several times in the Codex, is divisible by it. Can it be said that a conclusion based on no other evidence than this "amounts to a demonstration?"

That Dr. Förstemann has made progress in the study of the Codices by calling attention to the relations of these numerals to one another is cheerfully admitted, and that he has thrown light upon their meaning and suggested lines of investigation regarding them is undoubtedly true. Yet his discussion in the paper alluded to cannot be considered a "demonstration," when the same data may be used legitimately to lead to quite different results from those he obtains. The explanation which accords with the known Maya Calendar should be accepted in preference to that which requires a radical change, especially when that change is so radical as to wipe out the chief land-marks by which the Mayas were accustomed to reckon time.

Allusion has been made to the method of counting from the last day of the preceding month, — or, as Dr. Seler holds, commencing the months (and hence the years) with the days usually counted the last. Although not essential to the present discussion, we may say in reply to the suggestion which will arise in the mind of the reader, that the first method would necessitate beginning the count of the days from the last day of the preceding year, that this may furnish an explanation of what has hitherto been an unsolved problem — the numbering of the Ahaus. By counting in this way we can readily see why the first Ahau of a Grand Cycle or Ahau-Katun would be numbered 13.

NOTES ON THE FORMATION OF THE IRON ORES.[1]

BY JAMES D. ROBERTSON, E.M., JEFFERSON CITY, MO.

AMONG the deposits of iron ores in Missouri, those of specular hematite in the sandstones of the Ozark region, although not so well known as the porphyry ores, are an important and extremely interesting class. These deposits are found in caves in the Ozark series of limestones and sandstones, heretofore considered of Lower Silurian age, and the equivalent of the Calciferous sandrock of New York State. From recent investigations of the Missouri Geological Survey, there is reason to believe that these rocks are of Cambrian age.

At Cherry Valley, in Crawford County, there is a large deposit of specular hematite of this class. It occupies a depression in the strata formed by erosion and undermining. The ore is a good one for foundry purposes, and is smelted at the Midland Furnace, about six miles distant from the bank. The ore in the upper portion of the bank is harder and more silicious than that found deeper. It carries silica in the form of amorphous yellow jasper and also as quartz. The quartz is found in very perfect, singly terminated crystals, both colorless and amethystine, and of a dark, smoky color, the latter being due to included crystals and fragments of hematite. Specimens are in the Survey collection of perfect crystals of amethystine quartz, studded with crystals of specular hematite and frosted with minute acicular crystals of goethite of a beautiful golden-brown color. The ore has many vugs or cavities, which are lined completely with crystals of hematite, magnificently colored — red, golden-brown, peacock-blue, green, etc. While examining a number of these specimens lately, the writer's attention was drawn to the peculiar appearance of one, the markings of which had a great resemblance to the stem of a crinoid, while in another part of the same specimen there was a somewhat indistinct impression of a stem and cup of a crinoid. The specimen in question was submitted to Professor Van Hise of Madison, Wis., and by him to Dr. Birge, professor of zoölogy in the Wisconsin State University. These gentlemen say that there is not the least doubt in their minds of the organic origin of the peculiar markings of this specimen. Since then one more specimen of like nature has been found.

These organic remains, occurring in the deposit of specular ore just described, have a significance perhaps not wholly seen at first glance. They indicate that certainly a portion of the ore in these deposits has been formed by direct replacement, molecule by molecule of limestone by the iron from ferruginous waters. They also suggest that the chert, which is so abundant in these limestones, was the source of the silica in the ores. It is thus very probable that the iron was originally substituted as carbonate and subsequently concentrated and oxidized. The following terse statement by Professor Van Hise in regard to the deposit formed in a peculiar manner in Wisconsin is applicable here:[2] "The chemistry of the process assumes the following: that the oxygen of the percolating waters is sufficient to oxidize iron carbonate not in solution, and set carbon dioxide free; that the resulted carbonated waters are sufficient to take iron carbonate in solution; that if such waters bearing dissolved carbonate are mingled with waters bearing oxygen, the iron carbonate, or a portion of it, will be precipitated; that silica may be carried in percolating waters; that a carbon dioxide solution sufficient to precipitate silica by dilution may be made so weak in carbon dioxide that it would be capable of taking silica into solution. All of these facts and principles of chemistry are so well known that no discussion of them or reference to authority is needed."

One more question affected by these specimens is that of their age. While the fossil forms are too indistinct to identify the species, they are undoubtedly of Lower Carboniferous age. This suggests the idea that Lower Carboniferous rocks extended at least to this distance over the central portion of Missouri, and further that these deposits of iron ore had at least not ceased forming until after this period. These ore deposits, as stated before, are found in Cambrian rocks, but the precise period of their formation cannot be determined from our present knowledge of the facts in the case. So far, but one deposit of Lower Carboniferous rocks has been found in place anywhere near here, and that is a very small one. The writer has found fragments of chert with Burlington fossils on the hills some thirty miles west of here, but, with these exceptions, knows of no deposits of these rocks nearer than St. Louis County.

It is evident, therefore, that erosion has played an important part in removing the mantle of Lower Carboniferous rock, and it is probable that this agency at the same time prepared the underlying Cambrian rocks for the reception of the iron ores.

It is not within the scope of this article, however, to demonstrate the questions of the origin and age of these deposits, but merely to record the occurrence of these organic forms and to suggest their probable meaning. These questions are treated in a much fuller manner by Mr. Frank L. Nason, in the recently issued report of the State Geological Survey, on the Iron Ores of Missouri.

[1] Published with the approval of Arthur Winslow, State Geologist of Missouri.

[2] Am. Jour. Sci. (III), vol. xxxvii., p. 43.

NOTES AND NEWS.

AN important meeting of the Victoria Institute, London, England, took place last month, when Mr. J. W. Slater, F.C.S., F.E S., read a paper in which he traced the difference between life and the physical forces, and reviewed all those experiments and arguments by which some had sought to prove that a key to the origin of life had been obtained. Contributions to the discussion of the question were made by Sir George Stokes, Bart., V.P.R.S., who stated that Lord Kelvin's recently alluded to suggestion that the germs of life on this earth might have come from the bursting of a remote star, was only intended by him to refer to the possible transmission from one part of the universe to another of life germs, but that the first origin of life itself we must all refer to God. Professor Lionel Beale, F.R.S., in supporting Mr. Slater's views, said that an absolute line must be drawn between the living and the non-living. Living matter was distinguished from all other matter by a property, power, or agency, by which its elements were arranged, directed, and prepared to combine according to a pre-arranged plan for a definite purpose. There was no gradual transition from the non-living to the living. Life was a special position independent of and not in any way related to the physical forces, it had nothing in common with any material forces, powers, or properties, and holding in the cosmos a remarkable and peculiar place. Professor Bernard of Dublin pointed out that all evidence went to show that vital forces are unique and not comparable with any other forms of energy. Dr. Rae, F.R.S., contributed some valuable remarks, as also did Dr. Biddle, the Revs. R. Collins, M.A., J. H. Clarke, and W. A. Pippet. Dr. F. Warner, M.D., F.R.C.P., made several valuable remarks on the question, which was also spoken on by Dr. Shettle of Reading, Dr. Schofield, and others. Dr. Schofield was very interesting in those remarks in which he pointed out what may be called the history of the controversy in regard to life and the physical forces, and in concluding he specially referred to the dictum of Professor Huxley, viz., "Life existed before organism and is its cause." What that cause was the Christian philosopher fully recognized.

— J. B. Lippincott Company announce for immediate publication a new (third) edition of the "Life of Benjamin Franklin," edited from original manuscripts and from his printed correspondence and other writings, by Hon. John Bigelow. Since the appearance of the previous editions the author has been able to secure considerable new and important information never before published, which is incorporated in the new work. This edition also contains several additional interesting illustrations. The work is bound in three volumes, as heretofore. A new edition of "Our Own Birds" has just been issued by J. B. Lippincott Company. The volume contains a natural history of the birds of the United States, revised and edited by Edward J. Cope, Corresponding Secretary of the Academy of Natural Sciences, Philadelphia. Although the first edition was profusely illustrated, twelve new half-tone plates have been introduced, which greatly add to the value of this edition.

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AN OLD VOLCANIC ERUPTION IN IOWA.

BY CHARLES R. KEYES, DES MOINES, IOWA.

IN the extreme northwestern corner of Iowa there is a small area of crystalline rocks commonly known under the name of the Sioux Quartzite or Sioux "Granite." These are the only strata in the State showing any decided traces of being changed through dynamic influences. Everywhere else within the limits of the province the rocks are so horizontal in their position, so undisturbed by mountain-making forces, and so unaltered in lithological characters, that it is generally taken for granted that all the strata in the State are sedimentary in origin and repose essentially as they were originally laid down in the waters of the great interior sea which once occupied the heart of the American continent.

The Sioux quarzite is a hard, vitreous mass with undulating bedding planes. Its geological age is regarded as much greater than that of any other formation in Iowa; not excepting even the old Cambrian sandstone of the northeastern portion of the State.

Although the area of the Sioux quartzite is quite extensive, no other crystalline rocks have been noted in the neighborhood until very recently. It is, then, of considerable interest to know that Professor G. E. Culver has lately discovered in the midst of the Sioux quartzite, of southeastern Dakota, in Minnehaha County, within three miles of the Iowa boundary, a large mass of trap, which extends for more than a mile along one of the tributaries of the Big Sioux River. A microscopical examination of these rocks shows it to be a well-pronounced, coarse-grained, olivine diabase, with such minerals as hornblende, black mica, and apatite present in addition to the feldspar, augite, and olivine.

The presence of this massive basic rock of unmistakable eruptive origin is very suggestive of the agencies that have been at work to some extent in changing the old sandstone. Further investigations will doubtless disclose other similar types of intrusive rocks in the Sioux quartzite in all three of the States already mentioned.

But the occurrence of this black trap rock, which has undoubtedly been cooled from a molten condition, is made even more interesting by other discoveries of still more recent date. During the past few years a number of deep wells or borings have been made at different places in northwestern Iowa. The depths reached are from 1200 to 2000 feet. Several of these borings are of special interest, inasmuch as they pass through all of the sedimentary rocks into the crystalline beds below, penetrating them in some cases to the extent of several hundred feet. A typical gray granite has been recognized in some instances; in others different types of eruptive rocks. One of the latest borings in this part of the State is the well at Hull, in Sioux County. At a very considerable depth a number of beds of flint-like rock were passed through. The different layers were separated by sand and gravel several feet in thickness, if the records are to be relied upon. Some of the flint-like fragments were sliced by Mr. S. W. Beyer of the Iowa Agricultural College, and upon microscopical examination proved to be what is known to geologists as quartz-porphyry — a truly igneous rock or lava, very acid in character, and essentially identical with granite, but cooling under somewhat different physical conditions.

The presence of the several sheets of quartz-porphyry, which are to be regarded as different lava flows, show conclusively that volcanic forces were very active in northwestern Iowa in ancient times. The position of the lava beds seems to indicate, as will be pointed out by Mr. Beyer in his discussion of the subject in the forthcoming Annual Report of the Iowa Geological Survey, that the flow of the molten rocks probably took place toward the close of the Carboniferous age, immediately after the coal of the Mississippi basin had been deposited.

Mr. Beyer puts forward, therefore, two explanations: —

1. That the flow took place during palæozoic time, perhaps in the Carboniferous, the lava being secularly poured out over an old sea-bottom.

2. That, as a whole, the different flows were contemporaneous and in point of time post-Carboniferous. In this case the intercalations are to be regarded as the results of the subterranean lava flows — the lava following along lines of least resistance and flowing between the strata.

It makes little difference which of these two views is accepted, for certain it is that here in northwestern Iowa there is every reason for believing that there were at one time active volcanic agencies at work not unlike those seen to-day in southern Europe, around the shores of the Mediterranean Sea.

THE PERMIAN IN PRINCE EDWARD ISLAND.

BY F. BAIN, NORTH RIVER, P. E. ISLAND.

THE study of the Permian in North America hitherto has not been satisfactory. The areas studied west of the Mississippi and in Virginia exhibit the lower part of the formation which in organic remains so closely resembles the Upper Carboniferous that a clear and satisfactory periodic distinction is not observable. In the Gulf of St. Lawrence, however, where a long-continued and regular subsidence marked the close of the palæozoic, we have a perfect series of the Permian strata, three thousand feet in depth, recording the gradations of life in this district between the close of the Carboniferous proper and the beginning of the Mesozoic.

The Island of Prince Edward, in the southern part of the Gulf of St. Lawrence, is composed of red sandstones and shales, mostly Permian, capped in the central district by a denuded fragment of the Trias. Where these Permian beds stretch across the Northumberland Strait and appear on the coast of New Brunswick, they are seen to repose unconformably on the Carboniferous. Here the distinction between the two formations is very apparent. The Carboniferous is a coarse, gray marsh deposit, bearing numerous remains of *Calamites* and *Cordaites* and a few *Lepidodendra*. The Permian consists of fine, red marine deposits, bearing as their characteristic organisms *Walchia*, *Tylodendra*, *Baiera*, *Pecopteris arborascens*, and *Calamites gigas*.

In the lower part of the Permian the flora has marked Carboniferous affinities, but there is always a clear and distinct difference. On St. Peter's Island, for example, there is a marsh deposit of the Permian. The gray and brown sandstones and gray bleached clays contain but few calamites, and these of small size, except the giant *C. gigas*. *Cordaites* is also inconspicuous, but remains of *Tylodendra* and *Walchia* are in great profusion, and *Annularia* frequent. At Gallas Point there is the same abundance of *Tylodendra* and *Walchia* with *Dadoxylon* and *Pecopteris*, and here, as in the other localities, *C. arenasceus* begins to take precedence of the older Carboniferous calamites. At Mimimigash is an extensive fern deposit in red clay shale. *Pecopteris arborascens* is abundant and is in magnificent development. Its great, heavy fronds are seen nine feet in length, and its features rich and well developed. *Alethopteris nervosa* is common, but *Sphenopteris*, *Neuropteris*, and *Cyclopteris* are sparsely represented. *Annularia* is abundant. *Cordaites* and *Calamites* hold a minor

place, but *Equisitum rogersii* is in magnificent development, and branchlets of *Walchia* everywhere in abundance.

In the Upper Permian about Charlottetown, the Carboniferous features of the formation are almost lost. *Dadoxylon*, *Tylodendron*, *Walchia*, *Palissya* and *Baeria* mingle with *Voltzia*, *Pterophyllum*, *Podozamities*, *Clathropteris*, ferns of Mesozoic type, and abundant *Equisetacæ*.

At Carleton a bressiated conglomerate contains many osseous fragments of considerable size, which in structure have a reptillian aspect.

This series of deposits appears to have closed in an important glacial period, for on its summit rests not only drift fragments, which must have come from the distant hills of New Brunswick, but a well-marked glacial moraine, now consolidated into a firm mass of conglomerate five hundred yards in length, occurs in the valley of the Mill River, reposing on the summit of the Permian and underlying the horizontal Trias.

The Trias contains no good deposits of plants, but such remains as we find are quite distinct from those of the underlying formation. Even the ubiquitous *Walchia gracilis* has disappeared and a new form taken its place. *Voltzia*, *Palissya*, *Baiera* mingle with a few inferior *Cycads*, and the accumulations of the ancient sand reefs are everywhere penetrated by the repent stems of *Equisetæ* and their peculiar bulbous nodes.

This meagre flora is but the representative of Mesozoic plant life when the district was recovering from the desolation of a great glacial period. Later deposits are entirely wanting, but the chance occurrence of a high-typed Mesozoic reptile, the *Bathygnathus borealis* (Leidy), in these early beds, clearly establishes their systemic standing. The whole of this series of deposits is exceedingly interesting as illustrating the transition of plant life from the Carboniferous to the Triassic.

NOTES ON THE WING-COLOR OF NORTH AMERICAN LOCUSTS BELONGING TO THE SUB-FAMILY ŒDIPODINÆ AND ITS SEEMING RELATION TO CLIMATIC CONDITIONS.

BY LAWRENCE BRUNER, STATE UNIVERSITY, LINCOLN, NEB.

ONE of the many features that have been noted in the study of our North American locusts during the past ten or a dozen years is the color-variation of the wings of the different species of locusts of the sub-family Œdipodinæ. As all students know who have had anything to do with these insects, some have yellow, others orange, still others red, and a very few have their wings blue. While this is true, perhaps it has not been generally noted that the presence or absence of humidity seems to have some influence upon these color-variations in the different representatives of this group that are to be met with throughout the country. That such must be the case, I think there can be no doubt. But little investigation is necessary to show that along the Atlantic slopes and even in the interior of the continent as far westward as the eastern edge of the great plains, red or orange is the characteristic color. On the plains and in other arid districts of the west and southwest the red and orange give place almost entirely to yellow. In the mountains red re appears, while at a certain elevation and under peculiar conditions blue takes the place of both. In some species we find both red- and yellow-winged individuals. There are also those in which yellow- and blue-winged individuals occur. Nor are these wing-color variations confined strictly to special genera. We find both the red and yellow appearing in species of Arphia, Hippiscus, Derotmema, Trachyrhachis, Psinidia, Lactista, Tomonotus, Dissosteira, etc.; while the blue and yellow are common to representatives of Leprus and Trimerotropis.

We find the red-winged species most common in humid regions, the yellow-winged in more or less arid regions. In the United States the blue-winged forms are found chiefly in mountainous regions just between the dry and wet conditions. At Pueblo, Colorado, *Leprus wheeleri* occurs with either blue or yellow wings. Near Ogden and Salt Lake City are found both this species and *Trimerotropis cyaneipennis*. They occur most abundantly a little below the upper shore-line of the ancient Lake Bonneville, and from that point up and down the mountain slopes for several hundreds of feet. Below there are to be found yellow-winged species of Trimerotropis, above red-winged Arphias. Blue-winged locusts are also to be met with on the lava beds of the Snake River Plains, on the alkali flats of portions of Montana, Wyoming, Nevada, and California, and in the Coast Range of mountains in southern and Lower California.

This same variation in wing-color among the representatives of the sub-family was also observed in Mexico, where the writer had an opportunity of visiting a number of different regions from which specimens were secured. The dry interior contained most yellow-winged and the humid "tierra calientes" furnished most red-winged species; while the midway mountain regions were the characteristic home for a blue-winged locust.

The following species are found with both red and yellow wings, viz.; *Hippiscus tuberculatus*, the prevailing color red, but in the Big Horn Mountains of Wyoming yellow-winged specimens are not uncommon. *Hippiscus*, here in Nebraska, seems to furnish about an equal number of specimens of each color. A couple of others of the genus are know to have the same wing-variations. *Psinidia sucerata* in the East is normally red-winged, but in the West is yellow-winged. Two of our Arphias, at least, have either red or yellow wings, while *Trachyrhachis pardalina* may be either the one or the other — the red being most common eastward and the yellow-winged westward upon the plains, and red again in the Sierra Nevadas.

So characteristic does this variation in color of the hind wings of these insects appear, that I have about come to the conclusion that an examination of a fair representative collection of these insects would be a sufficient index of the climate of the region from where they came. Possibly I may be wrong. If so, I would be pleased to hear the views of others who have made this feature more of a study than I have.

CURRENT NOTES ON ANTHROPOLOGY.—XXIV.

[*Edited by D. G. Brinton, M.D., LL.D.*]

The Problem of Life.

"LE Probléme de la Vie" — such is the title of the latest work of that thoughtful and learned writer, the Marquis de Nadaillac. The great and serious theme he has chosen is handled with a masterful acquaintance with facts and a severely critical spirit. The sweep of his horizon is most extended. He begins with a statement of the possible methods of formation of the terrestrial globe, the first appearance of organic life upon it, and the succession of animal and vegetable organisms which have one after another occupied its surface, down to the beginning of the quaternary period. These questions fill about one-half of the three hundred pages of the volume. The remainder is an anthropologic study. The antiquity of man, the growth of his physical powers and intellectual faculties, and his identity throughout all ages, are the points which receive especial consideration.

The results of this long and patient research are unfortunately negative. "We must resign ourselves to the avowal that science can teach us nothing either about the first appearance of organized beings on the earth, or about their succession in time, or their rapid multiplication in space" (p. 176). "I look as vainly down the vista of the unmeasured past as I do in the present for any positive evidence of the evolution of organisms or the transformation of species" (p. 178). "As far as we wander, as widely as we search, everywhere the individuals of each species reveal the same uniformity of action, the same psychical fixity." Man alone shows the power of indefinite progress. "Before such facts, who will pretend that man and beast ever sprang from one common ancestry?"

Such is the author's conclusion.

The Early Iron Age in Central Europe.

With the general employment of iron, a new era arose in central Europe, one which gave birth to that high culture which has since focussed there the civilization of the world. An intense

interest, therefore, surrounds this remote period. History is silent about it, and archæology alone can guide us. This wondrous science reveals two diverse civilizations in that area during the early iron age, separated probably rather by a few hundred years of time than by a few hundred miles of space.

The first is represented by the remarkable cemetery of Hallstatt, near Salzburg. This locality discloses a people skilled in working bronze, gold, and iron, manufacturers of richly decorated and gracefully formed pottery, lovers of ornaments of amber, glass, and agate, and accustomed to cremate their dead. We may place them 500–800 B.C.

The late iron age is the La Téne period, one or two centuries before the Christian era, deriving its name from a station in western Switzerland. By that time the working of iron had reached a singular perfection; glass, gold, silver, and precious stones were frequent; the dead were buried in stone coffins, and a local coinage was for the first time issued in metallic pieces, now popularly known by the name "rainbow-keys."

Recent studies on this period are those of Dr. Jakob Heierli of Zurich, in the December number of the Proceedings of the Vienna Anthropological Society, who describes a La Téne station in eastern Switzerland; one by Dr. L. Niederle, in the Report of the International Congress of Pre-History at Moscow, discussing the age of iron in Bohemia; and an address by Von Troltsch before the German Anthropological Society with reference to it in southern Germany.

Enigmatical Stone Implements.

In *Science*, Jan. 6, Mr. Walter Hough describes a form of polished stone implement with grooved surfaces, and suggests that these utensils were employed in beating out fibrous bark for clothing, paper, etc. This suggestion is not improbable, and has been accepted by some curators. In the Trocadero Museum, Paris, these stones are labeled "Armatures de maillet à battre les fibres d'agave." In the University Museum, Philadelphia, one bears the label, "Pounder said to have been used in pounding the agave in making pulque." There is no doubt of the correctness of this identification. The Mexicans called these implements *amatequini*, paper beaters, from the verb *amauitequi*. Mr. Hough is also right in surmising that the Mexican paper was not made from the agave alone. Other materials were the bark of the "Cardia," a tree of the family Boraginaceæ, and palm leaves, *hojas de palma*, which Boturini says made the finest of all. An article on the *amatequini* may be found in *La Nature*, Dec. 15, 1888.

Another strange implement or ornament is the stone yokes or collars which are found in eastern Mexico. In the *Internat. Archiv für Ethnographie*, 1892, Dr. Ernst of Caracas has an interesting article on these. He believes them to be memorial tokens of great individual achievements and worn as signs of power and dignity, on certain ceremonial occasions. Mr. Strebel, who wrote an article some years ago on the same subject, entertained a similar opinion. As they are quite heavy, often weighing about sixty pounds, some have supposed they were intended to fasten the victim to the sacrificial stone, the *techcatl*. They are evidently not adapted for this, however. I would suggest that they were the stones used in the game of ball, *tlachtli*, described by the early writers, enclosing the aperture through which the ball was to be driven. Some are closed with an armature, one of which is figured by Dr. Ernst. They are to be distinguished from the stone yokes from Porto Rico.

Recent Researches in South American Ethnology.

South America offers as large an unexplored region as Africa, and one with as promising possibilities. Strange that it has not attracted more attention from adventurous travellers! One of these, M. Henri Coudreau, has accomplished three expeditions, at the instance of the French government, into the far interior of Guiana. His general results have appeared in various works, as "La France Equinoxiale," "Chez Nos Indiens," etc. Lately, his linguistic collections have been edited by the competent hand of M. Lucien Adam, in a volume forming Tome XV. of the *Bibliothéque Linguistique Américaine*, published by Maisonneuve, Paris. It contains ample and carefully prepared vocabularies of the Ouayana, Aparai, Oyampi, and Emerillon dialects The first two are shown on abundant evidence to be members of the Carib stock, while the two latter are Tupi dialects.

Ernesto Restrepo Tirado is a young and active archæologist of the Republic of Colombia, equally enthusiastic in field-work and in historical studies; as is well shown in his "Estudios sobre los Aborigenes de Colombia," the first part of which, a volume of 180 pages with a good map, was published in Bogota last year. It begins with an extraordinary list of the tribes who occupied the territory at the time of the conquest, largely drawn from the epic of Juan de Castellanos. That Mr. Restrepo had the courage to read the 110,000 verses which compose this epic is reason enough to entitle him to our profound respect. Of course, a great part of his study refers to the Chibchas, who had the highest culture of any Colombian tribes. They were, however, not the most skilful workers in gold. This honor belonged to the Quimbayas, upon whom he has written a long essay, separately published. As their wealth led to their early and complete destruction by the Spaniards, their ethnic affinity has not yet been determined.

The University of Zurich possesses the rare treasure of five skeletons of members of the Alakuluf tribe of Tierra del Fuego. It seems these wretched islanders were taken to Europe to show in museums, and by some strange fatality all died at Zurich of pneumonia. Dr. Rudolph Martin has worked up their osteology and published his results in the *Vierteljahrsschrift der Natur. Gesell.* in Zurich. He finds the skulls well shaped, mesocephalic, with relatively large cubical capacity, 1590 cubic centimetres, and the horizontal circumference greater than that of the modern Parisians, as reported by Broca. The torsion of the humerus was less than in Europeans, and two of the humeri showed perforation of the fossa of the olecranon. The study is an exact and an interesting one.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

A Reply to Professor Hathaway.

I HAVE just read the note of "praise and criticism" on my books by Professor Hathaway in *Science* of Feb. 17. Kindly allow me a few words in the way of reply. Passing over the first part of his note, and thanking him for any praise of my books which he has given them, I come to what he calls his "illustration of my treatment and use of the method of infinitesimals." He says: "Thus, by trigonometry,

$$\begin{aligned} \sin(x+dx) + \cos(x+dx) &= \sin x \sqrt{2}\cos\left(\frac{\pi}{4}+dx\right) \\ &\quad + \cos x\cos dx + \cos x \sin dx \\ &= \sin x + \cos x + \cos x\, dx, \end{aligned}$$

since $\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right) = 1$, $\cos dx = 1$, $\sin dx = dx$,

Hence $d(\sin x + \cos x) = \cos x\, dx$, a false result."

Of course, it is a "false result"; who would expect anything else when the work in it is false? But this is Professor Hathaway's work; not mine. His statement, made above, that "$\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right) = 1$," is *not true*. For,

$\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right) = 1 - dx$, as any mathematician can see.

Therefore, $d(\sin x + \cos x) = \cos x\, dx - \sin x\, dx$, a *true result*.

Professor Hathaway has given the above illustration, as he says, to show how I "establish the differentials of the trigonometric functions"; though I should have never known it if he hadn't told me; and I deny that I should ever have taken this roundabout way. I hope that Professor Hathaway will not give the credit of his "false result" to the infinitesimal method, which he says "is at best a dangerous one, even in the hands of the masters, let alone the average student." I think, on the contrary, that the method is a safe one, when well understood. "In the hands

of the average student" any method is dangerous. In view of Professor Hathaway's illustration, I do not feel called upon yet to "revise my eulogy on infinitesimals." E. A. BOWSER.

Rutgers College, New Brunswick, N. J., Mar. 2,

A Question of Evidence.

IN a recent number of *Science* I ventured to express the hope that a new era was dawning in American archæologic science, and that the department of geologic archæology especially would experience a needed renaissance. I laid particular stress upon the deceptive and meagre nature of the evidence already on record and ventured to point out the demands of the future with respect to certain lines of research. Some of my statements relating to the character of the evidence have given rise to sharp comment on the part of defenders of the paleolithic theory. I strongly deprecate personalities in scientific discussion and hesitate to refer in a critical way to the legitimate work of other investigators, desiring to restrict myself to such criticism as is absolutely necessary for sifting the evidence and getting at the truth; but the generalized statements by means of which I attempted to describe the old archæology are not sufficiently trenchant to be effective; more definite and detailed characterization must, it seems, be given. This can best be accomplished by means of illustrations drawn from the writings of those defenders of the faith who make most vociferous claim to superiority of knowledge and profundity of research. Numerous illustrations are at hand, but I will refer only to the work of those who have unfairly reviewed or offensively referred to the positions taken by me. Attention has been called in Professor Wright's work, "The Ice Age," to a number of papers bearing on the paleolithic question, written by Mr. H. W. Haynes of Boston. In these papers, twelve in number, I have carefully sought references to original observations on the glacial archæology of this country, and find to my surprise that they are limited to two lines and a quarter of text. These lines include, also, reference to the discoveries of Professor Wright, Dr. Abbott, and two others present on the occasion. The record reads as follows: "Several *implements* were taken by the others, *either from the gravel, or the talus* on the river bank, in my presence, and *I found five myself*."[1] The italics are my own, and call attention to essential features of the finds and to the fact that Mr. Haynes's investigations are expressed in five words — quite sufficient no doubt for the presentation of the matter, since the articles found were probably all modern pieces from the talus. Now, any one could find these objects in the talus at that day, and no one now attaches any value to such finds save three or four advocates of the paleolithic theory in America who hesitate to acknowledge, or fail to see the shortcomings, of their early work. The chances are a hundred to one that all talus finds and all the finds made by Mr. Haynes are Indian shop-rejects left by native workmen who utilized the argillite bowlders and masses that outcropped in the face of the bluff. But whether they were from the talus or not, I would call attention to the fact that the language used by Mr. Haynes in describing the discoveries indicates practical "ignorance" of the only essential points of the discussion of fossil man. In the first place had he known that the things he picked up "either from the gravel or the talus," as he states it, correspond exactly with the ordinary modern quarry and shop-rejects of the Trenton region, he would certainly not have ventured to class them with European paleolithic implements and to build a monument to American antiquity and to himself upon them; and, in the second place, had he known that the only legitimate proof of the antiquity of such specimens in America is geologic proof, he would not have failed to properly discriminate between those articles obtained from the gravels in place — if there were such — and those obtained from the talus. From his language it is evident that at that time he had no comprehension of the real problems involved, and could not have appreciated the necessity of the discriminating observation now considered essential by scientific men; consequently, his observations made in archæologic obscurity and geologic darkness amount to naught, and no subsequent patching-up can redeem them.

[1] Haynes, H. W. Proc. Boston Soc. Nat. Hist. Vol. XXI., p. 132.

Professor Wright, who is vigorously championed by Mr. Haynes, does not claim to have found any relic of art in the gravels, and hence probably knows nothing, from his own observation, favoring the glacial age of man in America, and I was led, in a review of portions of his published work, to question his judgment in writing so much on the finds of others, and accepting all statements that came to hand without apparent attempt at discrimination. Mr. Haynes has been more successful in his finds, having added five unverified turtlebacks to the long list of "paleolithic" strays. He may not have broken Professor Wright's record in number of papers published, but he has been less discriminating in the use of unsound data. Having little knowledge of native art and less of geology, he has rarely touched the subject of glacial man without adding to its obscurity. His most pronounced shortcoming is, however, in the line of original research; when the three lines recording his complete achievements in the American field are cut down to five words, as quoted above, and these words reduced to their *real bearing* upon the question of glacial man in America, we have only the punctuation left! It would be difficult to find within the whole range of scientific writing three lines containing less of science or evincing a greater degree of incompetence to treat of the subject discussed, than these.

Another example of "that half wisdom half experience gives" may be cited. In a recent publication, Mr. Haynes avers that I have rashly and wrongly characterized the work of other investigators; yet a hurried glance into his part of that work convinces me not only that I shall be acquitted of this charge, but that I may now safely venture farther. I am constrained, therefore, to suggest that perhaps Mr. Haynes's investigations of paleolithic man in Egypt — in the only field in which he can possibly lay claim to having added a single new fact of importance to the data of archæologic science — will not require more than five words for their proper record. A brief summary of these researches may be given.

Scattered over the surface of the ground in the valley of the Nile he found several implements of supposed St. Acheul type and numerous examples of other flaked objects of ordinary and extraordinary shapes. We learn, however, in his own words, that "Quaternary deposits do not occur in the Nile valley, so far as I am aware, though they have been found in various parts of the Sahara."[2]

The "implements" of St. Acheul type are assumed to be paleolithic because of their looks. This is the "evidence" of the ordinary paleolith hunter, and it does not appear of the least consequence to him that the quaternary deposits which alone could furnish the only real element of proof of antiquity — the geologic element — are not found in the Nile valley at all, but are said to exist somewhere in Sahara. These enormous leaps from meagre data to full-blown conclusions are characteristic of the past archæology, and awaken feelings of amazement in the minds of practical students to-day. Even if analogies of form in implements are allowed to have a definite value in cultural or chronologic correlations in Europe and adjoining lands, it must be insisted that in America, until types of flaked objects other than those found commonly in Indian shop-refuse heaps are established, the test of antiquity shall be a geologic test.

The two illustrations given serve to indicate my reasons for raising the question of competency with respect to the evidence relied upon to establish a paleolithic glacial man in America. Observations of the class cited, howsoever greatly multiplied, can never amount to proof, demonstrating rather the lack of it. My position with respect to this point need not be misunderstood: when a single artificial object is found that can be fully and satisfactorily verified geologically, I shall gladly join hands with other students in making it a nucleus about which to arrange all that are clearly fellows with it. Then, and not till then, will uncertainty become certainty, and not till then can the question of the grade of glacial art be taken up and profitably studied. I only ask that the evidence relating to glacial man be properly scrutinized, and that meanwhile paleolithic man in America shall bide his time.

[2] Haynes, H. W. "The Fossil Man," Popular Science Monthly, Vol. XVII., p. 358.

While awaiting the discovery of new evidence tending to establish a glacial man in America, I have undertaken to analyze the old testimony as embodied in the writings of investigators of the American questions, and short papers covering part of this ground will soon appear. I had not anticipated this present diversion, however, as I had thought of Mr. Haynes only as a convenient verifier of that large class of unfortunate "paleoliths" whose pedigree happens to be shaky. My work was intended to bear only upon that of real investigators, such as Abbott and Cresson and Metz, who have for years sought earnestly, if not always effectively, for the evidence that is to make symmetric the culture development of two hemispheres. Those writers who undertake to use, and defend the evidence collected by, these students, will do well to remember that they shine by borrowed light, and should for much-vaunted modesty's sake, if not for science sake, keep well within reach of its limited ray.

If my "rash" assertions, hitherto made, respecting the nature of the testimony relied upon to establish a glacial, paleolithic man in America, lead finally to a just estimate of the real evidence and to the establishment of a firm basis for future operations in this great field, I shall feel amply repaid, notwithstanding the storms of sharp words and the streamlets of doggerel the publication of these views seems destined to call forth.

W. H. HOLMES.

Washington. D.C.

The Neanderthal Skull.

IN reference to Professor Haynes's observation in *Science*, Feb. 24, p. 107, that, not having seen the report of Professor Virchow's address, he could not judge "how far Dr. Brinton may have been misled by his authorities," I beg permission to furnish both him and other readers of *Science* the opportunity of judging, by quoting Virchow's precise words about the place and surroundings of the Neanderthal skull. They are as follows: —

"Für die Beurtheilung dieser Gebeine ist es von Wichtigkeit zu erwähnen dass dieselben aus keiner Höhle herstammen; auch hat man sie nicht an ihrer Lagerstätte aufgefunden, niemand hat sie ausgegraben, sie sind in Bezug auf die geologischen Verhältnisse, unter denen sie sich befanden, nicht Gegenstand der Beobachtung gewesen. Sie wurden gefunden in einer Schlucht, die zunächst eines Bergabhanges sich gebildet hatte; durch diese Schlucht waren Wasser herabgekommen und hatten allerlei herausgespült; wo die einzelnen Stücke früher gelegen hatten, wusste niemand. Darunter befanden sich auch das Bruchstück des Schädels."

Professor Haynes refers to the finder, "Dr. Fuhlrott" (evidently meaning Fullroth). This person's statements are seriously questioned by Professor Virchow, apparently from information derived from Mrs. Fullroth, who imparted it in unsuspecting innocence of the grave decisions involved; as the Professor gleefully narrates. Virchow's earlier report will be found in the Verhand. der Berliner Anthrop. Gesell. for 1872.

D. G. BRINTON.

Philadelphia, March 1.

Aerial Bubbles.

THE account of "snow-rollers" in your recent issue recalls an atmospheric phenomenon which was beheld here by two witnesses of unimpeachable character several years ago, of which no account has ever been published. Towards sunset, late in April, 1886, on a warm, thawing day, the snow rapidly disappearing, two men, Capt. John E. Hetherington and Mr. Marcus Sternberg, as they rode down the long hill towards this village from the east, saw what appeared to be innumerable spherical bodies floating in the air like soap-bubbles. Both men saw and wondered at the appearance for some moments before either spoke. Capt. H. then said, "I wonder whether I am dreaming?" The other rubbed his eyes and echoed the sentiment. "Well," said the captain, "I wonder if you see what I see; what do you see?" They questioned each other, and both agreed as to their impressions. An orchard lay along the lower and northwesterly side of the road, and all in among the apple-trees were thick, gently-descending multitudes of these bubbles, pretty uniform in size, say, 8 or 9 inches in diameter, apparently; none less than six; no small ones being observed.

The two observers state that they carefully fixed their attention on particular bubbles, in order to compare notes, and saw them seem to rest on the bough of a tree, or the top board of the fence, and then gently roll off and disappear or go out of sight. The sun was sinking and dropped below the opposite hills as they reached the foot of the long descent and entered the village, and the appearance came to an end. But up to this time the air seemed to be filled with these transparent floating spheres. The position of the observers with regard to the light seems to have made some difference as to seeing well this or that large aggregation or swarm that one or the other pointed out. The bubbles were highly colored, iridescent, gave the same sort of reflections as soap-bubbles, and apparently vanished individually in much the same way. All these points I have ascertained by repeated conversations.

Captain Hetherington (Lieuteant Colonel by merit) is widely known for his extensive apiaries, the largest in the country, and is an exceptionally good observer. Mr. Sternberg also is a gentleman of intelligence and careful observant character.

The only theory I have been able to form to account for such a phenomenon is, that if a certain kind of dust floated off in the air, each particle composed of some sort of saponaceous envelope, enclosing a highly expansible centre or core, like ammonia, — particles of this character expanded by the warm air, and at the same time moistened, might, under very nice conditions, produce such an effect.

I will add, *apropos* of snow-rollers, that Mr. Sternberg states that, years ago, he once saw, in Schoharie County, what he called "auger borings" of snow; which he described as spiral rolls, about two inches in diameter, and broken into fragments of various sizes, like the borings turned out by an auger.

HENRY U. SWINNERTON, Ph.D.

The Parsonage, Cherry Valley, N.Y.

Hardy Towhee Buntings.

HAVING noticed the effect of the recent severe weather on the crows near Washington, which Dr. Ridgway gives an account of in *Science* of Feb. 10, I was greatly surprised to find the towhee bunting (*P. erythropthalmus*) evidently wintering here. During the second week in January last, I observed two individuals and heard the notes of others. As the towhee seems to get most of its food upon the ground, its presence during deep snows and severe cold rather surprised me. The authors of the U. S. National Museum Bulletin, No. 26 (Avi Fauna Columbiana), say of the towhee: "Chiefly a spring and autumn migrant. A few breed with us, but none remain during the winter." It usually makes its appearance here in the first warm weather in March, and I have found it to breed quite abundantly in suitable localities. During the same cold snap I picked up numbers of dead goldfinches, juncos, and native sparrows, evidently victims of the weather. The turkey vultures (*C. aura*) also suffer from the cold and are sometimes found unable to fly, their plumage being coated with snow and ice. In order to prevent the extermination of the bob-white during the past winter, a Virginia sportsman's club furnished quantities of wheat-screenings to any persons who would place the same in localities frequented by the birds.

ALBERT B. FARNHAM.

Bennings, D.C.

The Speech of Children.

THE paper in *Science* of March 3, having the above title, by Mr. A. Stevenson, has much interested me. In the fifth paragraph, on page 120, the author says: "The child apparently regarded himself only as object and not at all as subject." This conclusion is reached by the child's use of the third person in speaking of himself. It seems to me inconceivable that a conscious being should regard himself other than as subject. The peculiarity of expression — a common enough one in children — I believe to exist, first, because the child hears himself constantly referred to

as the object, and, second, because of the wrong and foolish method of conversation employed — not necessarily by the child's parents — when talking to him. Such examples as "Baby kiss mamma," "Does Freddie love his auntie?" "Is little Mary cold?" etc., can hardly lead to an early conception of correct verbal expression. HOWARD LILIENTHAL, M.D.

New York, 43 East 29th Street, March 6.

Solidungulate Pigs.

THE "mule footed hogs" inquired about by Mr. Jno H. Frick, in *Science* of Feb. 24, p. 107, are described and figured in my article entitled "On a Breed of Solid-Hoofed Pigs Apparently Established in Texas," Bull. U. S. Geol. and Geogr. Surv. Terr., Vol. IV., No. 1, Feb. 5, 1878. p. 295. ELLIOTT COUES.

Smithsonion Institution, Washington, D. C., March 1.

BOOK-REVIEWS.

Original Papers on Dynamo Machinery and Allied Subjects. By JOHN HOPKINSON, M.A., D.Sc., F.R.S. New York, W. J. Johnston Company.

THIS volume is a collection of the papers on electro-technical subjects which Dr. Hopkinson has published at various times during the last fourteen years.

It will be unnecessary to speak of the great value of these papers, for a number of them have passed into the text-books and form a part of the education of every technical student, and there is probably not an electrician in the country who has not found himself obliged to obtain the greater part of the remainder in some form or other. But a book of clippings from engineering journals is never so satisfactory as a bound volume, and the electrical profession will accord a warm welcome to this little book, the more so as it contains several papers which have hitherto been difficult to obtain. Of the eleven papers here collected, five are on electric lighting and dynamo-electric machinery, two on transformers and transformer tests, two on theory of alternating currents, one on an electrostatic effect in conductors carrying alternating currents, and one on electric light-houses. The first five contain the "epoch making" work on characteristic curves, and on efficiency tests of dynamos. (In passing, it may be noted that the paragraph on page 36, on the use of the characteristic to find the lowest speed at which a machine can be run and yet produce an arc, is given wrongly in Professor S. P. Thompson's "Dynamo-Electric Machinery." page 273.) But to technical readers the most interesting portion will be the papers on alternate currents and transformers, included in which is an account of the recent tests on the Westinghouse transformer, of importance as showing that the old accusation of poor all-day efficiency can no longer be made against the commercial transformer. These treat of the parallel and series running of alternators, the design of transformers, the effect of capacity in transformers, the power consumed in alternating current arcs, etc.

The advantage that this book has over the papers as originally printed is the fact that most of the errors and misprints have been corrected. A few yet remain, however. On page 155, 2μ should read 2π; *sine* $2\pi/T(t+\tau)$ should read *sin* $2\pi/T(t-\tau)$; the sign of the solution of the differential equation for H should be $-$ instead of $+$. On page 157, $e\gamma$ should read 2γ; through the whole of this part of the book H' is printed instead of $\dot{H}$. This would be objectionable if intentional, but it seems to be an accident, as on page 179 the dot is used instead of the stroke, but placed wrongly.

Electricity and Magnetism: Being a Series of Advanced Primers of Electricity. By EDWIN J. HOUSTON, A.M., Professor of Natural Philosophy and Physical Geography in the Central High School of Philadelphia. New York, W. J. Johnston Co.

FROM the preface we learn that this book is meant for the "general public" and the increased "number of those to whom a knowledge of the laws of electricity has become a necessity of every-day business life." While it is proverbially hard for a specialist to decide what the public want, it may be doubted if they will see much to choose between this and the scores of similar books which have been published. It is possible, however, that the name on the title-page may prove an attraction to many. On inspection the book is found to treat of the simpler theoretical principles, technical subjects, such as the dynamo, arc-lamp, etc., taking up about fifty lines out of the three hundred pages which comprise the book.

As in most books of the class, there are numerous inaccuracies; to mention a few: on page 23 a black surface is stated to be a worse radiator of light than a white one; whereas, of course, the reverse is the case; carbon is given as an exception to the rule that the conducting power of metals decreases with rise of temperature; the "conducting power of all alloys or mixtures of different metals" is stated to be "very much less than that of any one of the metals of which they are composed," in forgetfulness apparently of the fact that Matthiessen gives a long list of alloys whose conductivity is the mean of their constituents, etc.

The idea of giving references and extracts from books which should be read by those desiring a fuller knowledge of electricity than can be gained from the primers, can be considered a good one. It may, however, be questioned if the quotation from Professor Ayrton's book, "Practical Electricity," would give a reader the impression that it is a book on electrical laboratory work, and whether there is any necessity of quoting the author's "Electrical Dictionary" and "Physical Geography" so often among the selections from standard works, especially where, as on page 161, under "Extracts from Standard Works," the author quotes his dictionary as quoting Fleming, where the extract could, with no loss, have been made directly from the original. The chapter on Electrical Work is one of the best in the book, and the unscientific reader can hardly fail to understand the ideas treated of completely. R. A. F.

Contributions from the Botanical Laboratory of the University of Pennsylvania. Vol. I., No. 1.

Bulletin of the Scientific Laboratories of Denison University, Granville, Ohio. Vol. VII.

IN these days of enormous multiplication of books, magazines, journals, proceedings of societies, etc., there should always be reason for the establishment of a new serial. The avenues of publication are already so numerous that it is almost impossible to keep track of all. The agricultural experiment stations have vastly increased the amount of literature dealing with scientific results, and the comparatively new departure of universities, in issuing periodical publications, is one rather to be deprecated than encouraged. It would seem far better, for example, to do as Columbia College in New York, and Harvard University in Cambridge do, that is, to publish articles in established periodicals or scientific serials, rather than to originate new ones. Columbia College publishes the "Contributions from the Herbarium" in the Transactions of the New York Academy of Science, while Harvard University prints "Contributions from the Chemical Laboratory" in the Proceedings of the American Academy of Arts and Sciences.

These remarks are induced partly by the recent appearance of No. 1 of Vol I. of "Contributions from the Botanical Laboratory of the University of Pennsylvania" and Vol. VII. of the "Bulletin of the Scientific Laboratory of Denison University." Both of these are creditable publications. The former contains some valuable papers upon Dioncea and other subjects, and the latter is a catalogue of the flowering plants and ferns of Licking County, Ohio. With the Philadelphia Academy, the Franklin Institute, and the American Philosophical Society, all issuing serials in Philadelphia, the *raison d'etre* for a new serial there does not appear. The case of the Denison University is not quite parallel, but most probably there would be little difficulty in arranging for the publication of such papers in other places.

In the University of Pennsylvania contributions we have the following papers: "A Monstrous Specimen of *Rudbeckia hirta*," by J. T. Rothrock; "Contributions to the History of *Dioncea muscipula*," by J. M. McFarlane; "An Abnormal Development of the Inflorescence of Dioncea," by John W. Harshberger; "Mangrove Tannin," by H. Trimble; "Observations on *Epigæa re-*

pens," by W. P. Wilson; "A Nascent Variety of *Brunella vulgaris*," by J. T. Rothrock; and "Preliminary Observations on the Movements of the Leaves of *Melilotus alba* L. and Other Plants," by W. P. Wilson. Numerous new points are brought out by the studies of Dr. McFarlane on Dioncea. Among others he notes that two touches of the sensitive hairs are usually necessary to cause closure of the leaf. What he calls "memory power of the protoplasm," that is, response to a second stimulus when the first had no appreciable effect, he finds is sharply retained for from 30 to 45 seconds; and in from 55 to 60 seconds the effect of the first stimulus is lost. He also found that the hairs were not alone sensitive, although they were most so. But both outer and inner leaf surfaces show a marked degree of sensitivity. It was observed, likewise, that, although falling water, like rain, had little or no effect, immersion in water caused closure of the leaves as soon as the water touched the hair. Although three is the normal number of hairs on each blade of the leaf, our author has seen seven on one and six on the other half of a leaf; and he says leaves are frequently observed with from 8 to 13 hairs. "Such facts give countenance to the view that the sensitive hairs were once more numerous and diffuse in distribution, a condition still retained by *Drosera*." The hairs are jointed just above the base, and this seems to be the special irritable centre.

The epidermal cells of the leaves are stated to be admirable objects for observing the continuity of protoplasm. After proper treatment, the method being described, there are seen "along each side 18 to 30 protoplasmic bridges, which are slightly constricted on either side of the cellulose wall, and form a central swelling at the passage through the pore aperture. The transverse or oblique walls are traversed by 5 to 8 similar processes, so that the protoplasm of each epidermal cell is linked to that of neighbor cells by 50 to 75 fine connecting threads, and these again collectively are united with the cylinder of sensitive cells in the irritable hair." Various other subjects are considered, but they cannot be referred to here.

In the Bulletin of the Denison University, above mentioned, we have a catalogue of 945 species of plants occurring in Licking County, Ohio. Mr. H. L. Jones. the author, gives a list of the herbaria consulted, a short sketch of the county geology, and other facts. Among them are the times of flowering of the plants, and we note that in November 44 species bloom, in December 11, in January 14, in February 9, in March 17, and up to July 530. Thus no month of the year is without some flowers.

JOSEPH F. JAMES

Washington, March 1.

An Introductory Manual for Sugar Growers. By FRANCIS WATTS, F.C.S., F.I.C. London and New York, Longmans, Green & Co. 151 p. Ill. 8°.

IN the rapid extension of agricultural chemistry and scientific agriculture, a vast amount of tabular matter has been prepared, thousands of analyses have been made, and yet the results are neither satisfactory nor proportional to the work done. Professor Whitney has recently placed the position very clearly in saying, "There has been no satisfactory interpretation as yet of much of the work which has been done on the chemical composition of soils and plants, and the results of plat experiments have in most cases been very conflicting and uncertain." In this country the government experiment stations are issuing bulletin after bulletin of valuable and interesting reading; but even they, with all their superior advantages, have, as yet, fallen far short of their purpose. One reason for this is in the pre-eminence given to analysis and in the slighting of "condition," which latter feature forms a prominent part in the opening chapters of "An Introductory Manual for Sugar Growers," by Mr. Francis Watts, government chemist at Antigua, W. I. The first half of this interesting little book may be perused with profit by agriculturists the world over, presenting as it does a remarkably clear and intelligible dissertation on the elements of agricultural chemistry, treating first of soils, then of plant life and plant food, and finally of manures and fertilizers. The remainder of the work is devoted exclusively to the sugar industry, beginning with the planting and cutting of

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Mar. 7. — George H. Boehmer, Pre-Historic Naval Architecture of Northern Europe; George R. Stetson, Mental Atrophy in the Working Classes.

the cane, and passing on in natural sequence to the mill, the treatment of the juice, the manufacture of the sugar, the testing of the sugar solutions, and finally to the molasses and fermentation.

Throughout, the book is one for the practical man, and much important detail has been embodied within its pages. One of the best chapters is that treating of "Condition or Heart," plant food, and drainage, the first constituting, as Mr. Watts says, "a large portion of the science of agriculture." Analyses are, of course, given, but they are not asked to do duty for the whole, as is often the case in agricultural treatises. It is interesting to note, too, the remarks upon the assimilation of atmospheric nitrogen by the Leguminosæ, in which Mr. Watts follows Berthelot. Schlösing and Laurent have, indeed, recently denied the fixation of nitrogen by the action of microbes beneath the surface of the soil, but their theory of chlorophyll action needs far greater proof than they have offered. Berthelot is a good leader. Chapter III. deals with the sugar cane, treating of the preparation of the land, the manner of manuring and weeding, the cutting of the cane, and the utilization of the trash. Chapter IV. is of general interest, though the facts are applied in a particular manner to sugar growing. The collection, retention, and value of pen manures, the application of green dressing and of chemical manures, including potash, phosphates, superphosphates, sulphate of iron, etc., forms together an interesting study. The fallacy, which is so common, of supposed increase in manurial value of the excreta as compared with the food eaten, is here spoken of, as is also in a previous chapter the practice of burning the trash under the impression that thereby its value as a fertilizer is increased.

In the chapter dealing with the mill and the extraction of the juice, the various types of the former are compared and diagrams given. The application of hydraulic attachment to the rolls is mentioned, and a comparison is made of the results from crushing and those obtained by maceration and diffusion. The succeeding chapters treat of the juice, tempering, clarifying, filtering from the scum and the utilization of the latter, the manufacture of the sugar, in open pans, with strain, and in vacuum, and finally of the testing of the solutions and syrups. The production, composition, and uses of the molasses with the recovery of the sugar therefrom, and finally the nature of ferments and fermentation with the yield of alcohol and the forms of the stills employed, constitutes the subject matter of the concluding chapters. There are in addition tables of the temperature of steam at varying pressure, a list of the elements with their symbols and atomic weights, and a table of the densities, etc., of saccharine solutions.

We should be pleased to see this book in a second edition much enlarged and amplified, and trust that it is but the beginning of a series.

C. P.

AMONG THE PUBLISHERS.

SIR ROBERT S. BALL, F.R.S., the well-known English astronomer, has just completed an "Atlas of Astronomy," containing numerous beautifully printed telescopic views of planets, the sun's corona, etc., and diagrams of orbits. There are many star maps, and a series of twelve plates devoted to the moon, showing its aspects on consecutive days from the third to the fourteenth, making seventy-two plates in all. An introduction of nearly sixty pages gives a comprehensive explanatory text. The Atlas is published by D. Appleton & Co.

— Bulletin No. 40 of the United States National Mueum is No. IV. of the Bibliographies of American Naturalists, published by the government. This one is by L. S. Foster, and gives the writings of the ornithologist, Geo. N. Lawrence. A portrait faces the title-page, and in the course of the 124 pages, 121 titles are enumerated. Under these titles are given all facts connected with them. The species given in each are enumerated, together with the locality and page. A very full index gives ready reference to any species mentioned.

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For exchange.—Slides of Indian Territory Loup Fork Tertiary Diatoms for other microscopic fossils. Address S. W. WILLISTON, Univ. of Kansas, Lawrence, Kans.

For exchange.—Will exchange an "Ideal" Microscope of R. and J. Beck, London, 2 eye pieces 3 objectives, 3 inch, 1 inch, 1-6 inch; bull's eye condenser on stand, substage condenser, mechanica stage, etc., for any of the leading makes of type writers. Particulars by mail. DELOS FALL, Albion College, Albion, Mich.

Sale, or exchange for similar material; Diatoms (Isthmia nervosa), unmounted, from San Francisco Bay. M. J. ELROD, Bloomington, Ill.

For sale or exchange. — I have a few copies of my translation of "Strasburger's Manual of Vegetable Histology, 1887," now out of print, which I will send post-paid for $3 or for one dozen good slides illustrating plant or animal structure. Address A. B. Hervey, St. Lawrence University, Canton, N. Y.

The undersigned has the following specimens to exchange for crystals of any eastern or foreign localities or Indian relics: tin ore, metacinnabarite, stibnite, garnierite, calenanite, hanksite, ulexite, rubellite, lepidolite, blue and green onyx, Cal. pineite, aragonite on chalcedony, cinnabar, double refracting spar, clear and clouded, and others. J. R. Bush, care of General Delivery, Los Angeles, Cal.

For sale or exchange.—A private cabinet of about 200 species of fossils, well distributed geologically and geographically. Silurian, about 40; Devonian, about 50; Carboniferous, about 80; others, about 30. Frank S. Aby, State University, Iowa City, Ia.

Wants.

A COMPETENT TEACHER of botany in college or university is open to engagement. Address L., Box 86, Rochester, Mich.

CAN any one inform me as to the age to which cats have lived? I have one twenty years old. Edward D. Webb, 132 W. Eighty-first St., New York.

WANTED — Second-hand. Foster's Physiology, Balfour's Comparative Embryology, Claus & Sedgwick's Zoology, Flower's Osteology of Mammalia, Vine's Physiology of Plants. Please state editions and prices asked and address Richard Lees Brampton, Ontario, Canada

WANTED.—American Journal of Conchology, seven volumes. Parties having these for sale will please address the undersigned, stating condition and price. R. Ellsworth Call, Louisville, Ky.

A GRADUATE ENGINEER will give instruction evenings in geometry, trigonometry and surveying, mechanics, physics, mechanical drawing and general engineering construction. Five years' experience in field and editorial work on engineering journal. References furnished. C. S. H., 102 Tribune Building, New York.

WANTED.—By well-qualified and experienced science master and associate of the Royal School of Mines, London, aged 26 (at present in England), a mastership in technical college or university for any of the following subjects: Engineering sciences, geology and mineralogy, physics, chemistry and metallurgy, etc., etc. Can provide excellent references and credentials. Apply, J. G., 17 Sussex St., Rochdale, England.

A GRADUATE of the University of Pennsylvania and a practical mineralogist of twenty years' experience desires to give his services and a cabinet of 25,000 specimens, all named, with about the same number of duplicates, in minerals, crystals, rocks, gems, fossils, shells, archæological and ethnological specimens and woods to any institution desiring a fine outfit for study. The owner will increase the cabinet to 50,000 specimens in two years and will act as curator. Correspondence solicited from any scientific institution. J. W. Hortter, M.D., Ph.D., San Francisco, Cal., General P. O. Delivery.